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# MATHEMATICAL ECONOMISTS AND OTHERS: A PLEA FOR COMMUNICABILITY\*

By J. M. CLARK

MATHEMATICAL economics has a unique contribution to make to economics in general; and the object of this paper is to raise questions as to how this contribution can best be made. This is done from the standpoint of an economist whose mathematical equipment is distinctly rudimentary, by present standards, but who has tremendous respect for the accomplishments of the mathematical students, and a desire to make as much use of them as possible, or to see them utilized as fast as necessary verification is accomplished. But the difficulties are great, and the nature of the problem may not be fully understood.

It may be worth while to raise the question how mathematical economists themselves conceive the process of verifying and utilizing their results. These may remain as end products in the pages of *ECONOMETRICA* and similar publications. Or a few mathematical economists may be appointed to governmental positions, where they may propose measures, based on their results, to be passed on without understanding of this basis on the part of those who will make the decisions. The results would be far more widely useful if they became part of the common equipment of economists in general—necessarily a selected number of theorems, of tested validity and importance. But mathematical economists remain a growing and able sect, using an esoteric method and a special language, which make their results increasingly inaccessible to the rest of us.<sup>1</sup> So a plea for communicability seems in order.

The requisites are, first, that the general economist should be able to satisfy himself that the results have been verified; and secondly, that they should be put in a form that makes it possible for him to appraise their significance. As to the first, the general economist may be

\* This paper is an expansion of one section of the author's address on "Some Cleavages among Economists," given at the dinner meeting of the American Economic Association at Atlantic City, January 24, 1947, and published in the *American Economic Review*, Vol. 37, No. 2, *Proceedings*, May, 1947, pp. 1-11. By the courtesy of Professor Clark and of the Editor of the *Proceedings*, this important message to econometricians is brought to the attention of our readers.—Editor.

<sup>1</sup> The term "sect" may be objected to. A given mathematical economist may be other things besides; but in his special capacity he seems as much a member of a sect as a Methodist, who may be also a grocer or a Rotarian. This applies, of course, to the more elaborate uses of mathematical discourse, and to those who use it as a major vehicle of their thinking.

content to leave verification of algebraic processes to the mathematical jury. He knows that slips are possible, but may trust the specialists to catch them. For his purposes, the more important thing is verification (a) of concepts and premises, as to their resemblance to reality, and (b) of the consequent degree of approximation with which reality can be expected to conform to the indicated results. And this is a job for a different type of student from the one whose time has been spent in the mastering and manipulation of these high-powered techniques. Mathematical economics can fairly be asked, in the interest of its own influence and acceptance, to do its part toward making such verification possible.

The problem applies in differing degrees to three major areas of mathematical study. The first is the study of relations between tangible aggregates in the economy at large. Here mathematics is at its best, verification is most nearly practicable, and the difficulties of other methods are at their greatest. The second is the analysis of business reactions, assumed to be directed to maximizing profits. Here the assumption is indefinite in meaning, involving arbitrary limitations on the business man's expectations of consequences, which involve logical dilemmas, and involving also simplification of the environmental conditions. It is also seriously incomplete as an explanation of actual business behavior. The third is the analysis of individuals' choices between qualitatively different values, typically via indifference curves. Here the attempt to avoid psychology excludes major parts of the essential problem, while the remaining psychological implications, minimal as they attempt to be, are sufficient to do violence to the character of economic choices.

There are also algebraical and geometric expositions, the latter requiring functional relations to be given a shape. It is more communicable, also more limited. The instances that will be referred to in these comments are largely analyses of business decisions, also largely of the geometrical type.

There appears to be some lack of clarity as to what economists in general can justly ask of mathematical economics. They are not concerned with what kinds of rabbits the initiated can pull out of their curious hats—the widely-variant answers to the problem of duopoly may serve as a simple illustration of the different breeds of rabbits that can emerge from not-very-different-looking hats. And in a limited number of instances in which I have been able to check how the rabbit got into the hat, I have concluded that he did not have a very close biological relationship to the rabbits one meets in actual economic life. It would be invidious to cite instances; but those I have in mind include outstanding ones, which have earned rank as classics in their field.

What an economist wants to know is what features of the concepts and assumptions used are responsible for the character of the results, and how much difference it would make to the results if these concepts and assumptions were modified by taking in more of the complexities of reality. And when an optimum, or a level of equilibrium, is defined, he needs to know whether it is closely-determined, or whether it is of such a sort that a small range of indeterminateness in the conditioning factors could permit a large shift in the result. Sometimes it seems to be of the latter character.

What is needed is not a reduction of difficulty for the uninitiated by elision of steps in the reasoning; rather the contrary, it is greater attention to certain steps which the specialists tend to elide, when they are talking, as they usually must, to one another. The economist who will not, or cannot, take the punishment involved in following the logical process through, need be given only secondary consideration. But a mathematician, friend of the writer, taught him that the essence of mathematics, as a logical process, consisted in rigorous definition of terms and a deliberately pedestrian inclusion of all the steps. And this is necessary to one who would verify results of particular studies, without having followed the entire development of them in the literature, during which the initiated have become accustomed to taking many things for granted. So he is balked, not only by special uses of symbols, but by undefined terms and elided steps. Professor Hicks recently told the writer that he likes to test propositions by putting them both in language and in symbols. This seems highly desirable for important theorems, not only as facilitating verification, of the sort indicated, but as safeguarding the original reasoning against nonsense results. In manipulation, abstract symbols can be made to do things foreign to the nature of the economic realities they represent; hence symbols do not automatically eliminate loose thinking.

Bertrand Russell once spoke, I believe, of the ambiguity of the word "is." This presumably remains, but is harder to detect, when the word is translated into an "equals" sign in an equation. More concretely, the economic meaning of "supply" and of "supply schedules," as autonomous economic determinants, is peculiarly tricky; so also is the concept of substitution of factors for one another in response to adjustments of their costs. Essential factors of time are terribly easy to ignore.

In one case the writer was unable to find a concept of "supply" that would do what was assumed, except in a wholly-imaginary economy. In another case, the only conclusion he was able to reach was that the indicated result sometimes happened, and sometimes the opposite; and that when it happened, it did so for several reasons other than the one given. In another case, the conclusion was that a limited movement

occurred where the formula indicated an unlimited one, because the determinants—supply schedules—did not act as the formula assumed. In another case, what seemed to the writer an essentially long-term adjustment was imported into a study of cyclical movements.

If all cases were like these, one need not worry too much. The trouble is that it seems likely that this body of thought contains important truths—truths so important that they should not be ignored—mixed with other matter and not verified to the point of being safe to adopt and apply. And I take the risk of giving offense where none is intended, precisely because of my sense of the probable importance of some of these truths. In fact, this method seems able to make unique contributions to the whole long-neglected area of total flow of income and employment; and it would be tragic if the type of economist fitted for realistic verification of premises should be excluded from access to the most powerful theoretical analysis in this field. So my purpose is not needling, but rapprochement.

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